arteries where the action of the heart produces the whole effect," and adds: "There is much obscurity on the subject, as well as in the course of the blood in the general veins; and every judicious mind cannot fail to observe that there is a great vacuum to be filled up."

The object of this paper is to contribute to the solution of this mystery—first by proving that there *is* pulsation in the veins, and that therefore the heart's action *is* directly concerned in the return of the venous blood, and secondly, by suggesting the mode in which it is exerted.

I had observed that, on very close and careful inspection, there was in the veins (in those at the back of the hand, for instance) a visible though exceedingly delicate pulsatory undulation; but so minute that I have generally (but not always) failed to show it to others. It therefore became desirable to devise some means by which the fact might be made more manifest.

In the first instance I requested a lady, who was unable to perceive any pulsatory movement in the veins on the back of her hand, to feel and silently count her own pulse while I counted aloud the beats as indicated by what I could perceive in those veins. She was surprised to find that my counting corresponded exactly with hers, but observed that the beats of her pulse came intermediately and *alternately* with my counting. This I had not thought of or suggested. But it is, in fact, what would be the necessary result of the heart's action, and corresponds with its *alternate* contraction and dilatation.

In order to make more evident this venal pulsation the following experiment was successfully tried:—A small piece of silvered glass (about ⅜ inch by ¼ inch) was made to adhere to the surface of a swollen vein on the hand in such manner that *one* edge of the glass rested on the central ridge of the vein, while the other was in contact with the surface clear of the visible vein by the side of it. This, applied in the sunshine, of course afforded a spot of light, the movement of which reflected from the mirror, would indicate the slightest tilting of its plane by the undulating action of the vein, and the result was beautifully conclusive. The light spot vibrated in accordance with the pulse, and its vibrations were in the direction which corresponded with the tilting which should be the result of the position of the mirror in relation to the vein. Then a second mirror was applied on the *opposite* side of the vein, and the vibrations of the light spot took an *opposite* direction, which was to be expected, as the result of a tilting anticlinal to the first. A mirror placed on the knuckle (where are no veins of sensible importance) showed no sensible vibration. Thus the objection which I anticipated, that the vibration of the light spot might be caused by the general response in the limbs to the ordinary arterial pulsation, is answered by the evidence that a varied position of the mirror in respect of an individual vein was productive of a correspondingly varied motion of the light spot.

If, then, it is proved that, notwithstanding all previous authorities (to which I have had access), pulsation in the veins does exist, pulsation corresponding in rhythm with that of the arterial system, it becomes a corollary that the heart's action *does* extend to the motion of the blood in the veins, and an evident solution of the mystery of the return of the blood from the extremities appears to result. The expulsive effect of the heart's contraction is familiar, but the effect of its expansion, much in consequence of the venous pulsation having been unseen and denied, has been, as far as I know, ignored. Every one knows how an indiarubber ball syringe is filled by its expansion after compression. Apply this analogy to the expansion of the heart, and the return of the venous blood, the valves in the veins cooperating, would be equally certain. But this involves the existence of a corresponding venal pulsation, the supposed absence of which supported the theory that the direct action of the heart was limited to the arterial system.

I add some directions for the successful trial of the mirror experiment. The pulsatory motion is very small, and the action of neighbouring veins seems to cause parts of the surface to be neutral in respect of the displacement of the plane of the mirrors. It is, therefore, desirable to search experimentally for the best place for them; that is, where the resulting displacement becomes most evident; also the use of some sort of vigorous movement of the body or limbs, such as would cause a general exaggeration of the heart's action, naturally causes the vibrations of the light spot to become more conspicuous. The hand should be supported in the most steady manner, otherwise the pulsatory vibration becomes mixed up with an indefinite movement of the light spot due to general unsteadiness. The mirrors should not be more than ⅜ inch square or thereabouts. The silvering is liable to be detached from the glass by adhesion to the skin, if the glutinous substance is applied directly to the back of the mirror; to prevent this its back and edges should be covered with thin gummed paper (such as the margin of a sheet of postage stamps affords). This protects the silvering, so that the mirrors may be used repeatedly, and their position changed as often as may be required; whereas without this precaution they may be spoilt on the first application. Any sticky glutinous substance which does not dry readily (such as indiarubber dissolved in mineral naphtha) is convenient, because by its use the mirror may be with the least trouble shifted from one spot to another, in the search for a place where the venous pulsation is most visibly effective; and this will not always be found exactly where from the appearance of the veins it might be expected to occur.

J. HIPPISLEY

Stoneaston Park, Bath, August 10

The Fauna of the Sea Shore

IN the recent correspondence in NATURE on the "fauna of the sea shore," an ambiguity has arisen in the use of the term "littoral."

I, following Prof. Moseley, on whose lecture I was commenting, used the word in the extended sense of describing areas and faunas that were neither "deep-sea" (in the modern acceptation of the term) nor "pelagic." Mr. Hughes, on the other hand, has employed it in its common acceptation as descriptive of the shore area between tide marks.

The portion of the sea-bottom disturbed by waves has at present no term told off to describe it. It is not necessarily "littoral" in any sense, as that word will not cover the case of sand-banks far from the coast; such, for instance, as the banks of Newfoundland, where, according to both zoological and nautical evidence the waves act strongly on the sea-bottom. Some such term as "undal zone" might be used to describe those marine areas where the waves can sensibly affect the fauna.

The downward limit of this undal zone has not, I believe, been hitherto defined. In the case of oscillating waves (the ordinary ocean waves) 50 fathoms seems to approach the practical limit of disturbance, but, according to the evidence of marine charts, the waves appear to make themselves felt at greater depths.

In the late Mr. R. A. C. Godwin-Austen's map of the English Channel (*Q. J. G. S.*, vol. vi. p. 96), the following deposits are indicated, viz.:—

40 to 50 fathoms, fine granite shingle with fragments of *Haliotis tuberculata*.
50 to 60 fathoms fine granite shingle with fragments of *Patella vulgata*.
70 to 80 fathoms, coarse sand and gravel, with decayed *Patella vulgata*.
90 to 100 fathoms, coarse sand, fine gravel, *Cardium edule*, *Turbo littoralis*, and *Patella vulgata*.
Outside 100 fathoms, very fine shell sand, *Pecten varius*, *Cardium edule*, *Patella vulgata*, and *Turbo littoreus*.

Referring to one of these collections of shells (in upwards of 90 fathoms) between Ushant and the Little Sole Bank, the distinguished author remarks:—"Taking the two phenomena together, the occurrence of littoral shells and of marginal shingle, we may safely infer that we have at this place the indication of a coast line of no very distant geological period, buried under a great depth of water, and removed to a great distance from the nearest present coast-line."

The fact that shells are perishable owing to decay, corrosion, and the ravages of marine organisms, seems to me to militate against the probability that the shells in question are of geological antiquity; and their occurrence in connection with sand and shingle instead of mud would rather indicate the present action of currents strong enough to keep the sea-floor clean. This ordinary tidal currents cannot do, though wave and tidal currents combined can.

Under the joint influence of storm waves and storm-engendered currents, light shells may well travel down the channel bed to 40 or 50 fathom soundings. Theory and observation agree in the efficacy of wave and current to this extent. But to account

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for the presence of fresh littoral shells in 100 fathoms, we require the assistance of waves of sufficiently long period to affect the bottom at that depth, and to this extent theory in the case of ordinary ocean waves will not go.

In a paper submitted last year to the Dublin Society (*Proc.*, vol. iv. p. 241) I recorded observations of waves with an average period of 3½ minutes, and suggested that these waves arose from wind pressure on the surface of the sea; it would be interesting to know at what depth such very long, though irregular, waves would be capable of disturbing light deposits on the sea-bottom. In sheltered localities I have seen these waves attain the height of about three feet; in exposed localities they would doubtless be higher.

Arthur R. Hunt

August 15

On the Terminology of the Mathematical Theory of Electricity

In a letter (Nature, vol. xxxii. p. 76) Mr. W. J. Ibbetson invites suggestions for a convenient abbreviation for "total or resultant pressure"; at the same time he suggests the adoption of "traction" for "intensity of tensile stress." As it seems a pity to employ two totally distinct words to express such closely related ideas as intensity of tensile stress and total tensile stress, I would suggest that, on the analogy of pressure, "*tensure*" should be introduced for "intensity of tensile stress;" and then, on the analogy of "tension" for "total tensure," "*pression*" for "total pressure." New words are hard to grow in a language, but in this case pressure and tension might interchange their suffixes as grafts and yield two fresh useful words.

As regards physical and mathematical terminology in general, is not the time ripe for the introduction of a prefix which will modify the meaning of a term as the adjective "negative" does? *Mega* and *micro* have proved useful for multiplying and dividing by a million, but how much greater scope would there be for a prefix "ne" or "neg" for reversing the sign of a quantity. Thus negative electricity might be called "*nelectricity*," a quantity of negative electricity as so many "*necoulombs*," a negative magnetic pole as a "nepole," a negative potential as of so many "nevolts," a negative angle could be spoken as of so many "negradions" or "nedegrees," a negative temperature as of so many "nedegrees." In many cases there would be no appreciable advantage, but if there was a general understanding as to the operation of the prefix "ne" in any case, it could be used wherever it would render the phraseology less cumbersome.

Melbourne, July 10 William Sutherland

An Encysting "Myzostoma" in Milford Haven

I have recently had occasion to examine a number of *Comatulæ* from Milford Haven which were kindly given to me by Mr. W. Percy Sladen, F.L.S., and appear to belong to the type that was figured by Miller under the name of *Comatula fimbriata*; and I was surprised to find many of the pinnules presenting distinct traces of an encysting *Myzostoma*. In each of the dozen individuals the joints of one or more pinnules are abnormally developed, and in some cases they form definite cysts, which are, however, much smaller than those found on the pinnules of many *Comatulæ* and *Pentacrinidæ* from the Pacific and Oceania; but they are obviously of the same character and due to the presence of a parasitic *Myzostoma*. According to Prof. L. von Graff eight species of encysting *Myzostoma* are at present known, but they are limited to depths of 120 to 600 fathoms in the Pacific, the Eastern Archipelago, and the Caribbean Sea, with the exception of one which was dredged by the *Hassler* in 35 fathoms off Cape Frio, Brazil.

Mr. Sladen's dredgings at Milford, therefore, have considerably extended both the bathymetrical and the geographical distribution of these encysting species. The five *Comatulæ* found in the British area have yielded six of the free-living *Myzostomas*, four of which were discovered by the *Porcupine* and *Triton*; and we may probably take it for granted that the encysting form from Milford is another addition to the British fauna.

I propose to send all my material to my friend, Prof. von Graff, for examination; and as there will, no doubt, be much shore-dredging carried on during the next few weeks, I would call the attention of British naturalists to the facts mentioned above, and ask them to look carefully on the pinnules of any *Comatulæ* which they may find for cysts or other enlargements of the joints.

P. Herbert Carpenter

Eton College, August 22

Solid Electrolytes

In reference to Prof. S. P. Thompson's letter dated August 17 (Nature, vol. xxxii. p. 366), may I be allowed to say that I too have observed the secondary currents which are produced by cells containing sulphides of silver and copper after being disconnected from a battery? I mentioned the fact at the meeting of the Physical Society on June 27, in a communication which will probably be printed in the *Phil. Mag.* next month. Indeed, the observation of these secondary currents preceded and led to the construction of the primary cells with solid electrolytes which I have recently described.

I should be glad to know whether Prof. Thompson can explain the curious effect produced by passing a battery-current for a moment through a cell containing a mixture of sulphide of copper and sulphur between silver electrodes. When the cell is first connected with the galvanometer the usual secondary current appears, but in a few minutes, or even seconds, this current falls to zero and is succeeded by a third, which is in the same direction as the battery-current, and generally continues for some hours.

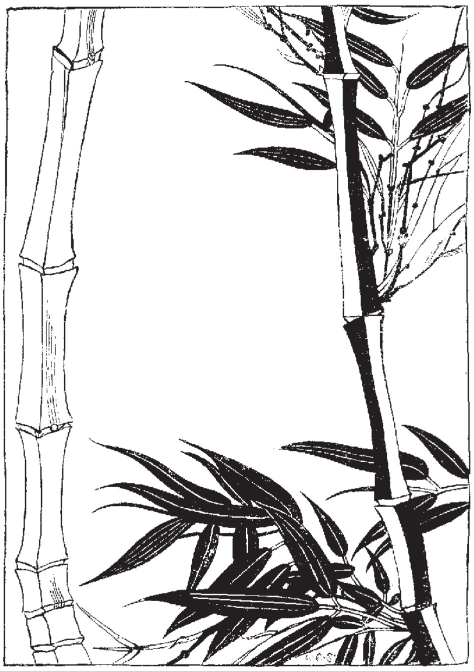
Shelford Bidwell

August 23

THE SQUARE BAMBOO

The cylindrical form of the stems of grasses is so universal a feature in the family that the report of the existence in China and Japan of a bamboo with manifestly four-angled stems, has generally been considered a myth, or, at any rate, as founded on some diseased or abnormal condition of a species having stems, when properly developed, circular in section.

Of the existence of such a bamboo there cannot, however, now be any kind of doubt. It is figured in a Japanese book, the "Sô moku kin yô siû" (Trees and shrubs with ornamental foliage), published at Kyoto in 1829, and the figure is reproduced by Count Castillon in the *Revue Horticole* (1876, p. 32).[1] It is further figured in a work for a copy of which we are indebted to my friend Prof. Kinch (formerly of Tokiyo), called the "Ju-moku Shiri-yaku"—*i.e.* a short description of trees (of Japan). Finally, in 1880, Messrs. Veitch presented to the Kew Museum fine specimens of the stem of the square bamboo,



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[1] The woodcut also appeared in the *Gardeners' Chronicle* for January 29, 1876, p. 147. I am indebted to the Editor for its use on the present occasion

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